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Clinical Lecture

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BY

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*Introductory ; with some Remarks on the Statements made in
a recent Review of Dr. Todd's work " On the Treatment
of Acute Disease."*

GENTLEMEN,—As this is the first Clinical Lecture which I am called upon to deliver this session, I propose in the first place to call your attention to one or two general points connected with clinical observation and the treatment of disease, and afterwards to allude briefly to the cases in the Hospital under my care. You are probably all aware that only a small number of patients are allotted to me ; but you must not therefore conclude that little is to be learned by watching them from day to day. Depend upon it, you will gain more real information by carefully studying one case thoroughly than by subjecting a great number to cursory examination. My class is, as you know, a voluntary one, and meets at nine o'clock. As the cases are limited, we shall have time to go into matters of

detail with reference to diagnosis, which are not less interesting than important.

You will, I think, find by and bye, that the degree of confidence you yourselves feel as to having pursued the right plan of treatment in a case, will be much influenced by the confidence you feel in having made a correct diagnosis. When inquiring into the nature of a case, do not try to find a name for the disease, and then ask yourself what physic is good for it; for if you pursue this course, you will never gain that sound judgment and discretion which alone will give you confidence in yourself, and enable you to gain the confidence of those who consult you.

When you examine a patient, try to make out everything in which he differs from a healthy man; and, if possible, ascertain the causes of the difference. See if his heart, and lungs, and liver, and intestinal canal, and skin, and kidneys, act as they should do in health; and ascertain if his muscular and nervous organs perform their offices properly. Make these inquiries in a definite order, and arrange the information you receive according to a well considered plan.

I shall endeavour to assist you in every way that I am able; and we shall from time to time go through the detail of taking a case, in order that you may have an opportunity of seeing the practical use of an artificial method, the object of which is to prevent us from omitting some important points which might be passed over if our inquiries were not made in a methodical order.

With regard to the subject of treatment, let me warn you against accepting this or that plan as the right one, because it is stated that a small per centage of deaths or a large per centage of recoveries is recorded in its favour. If you observe the cases which come under notice in the wards, you will seldom find two which exactly resemble each other in their origin, course, and results. We all possess certain individual peculiarities, require different quantities of food, are influenced

differently by the same causes, and, although we may be exposed to the same conditions, we shall not all be affected in the same way. It does not follow that, because we may be suffering from a similar disorder, we shall therefore progress favourably under precisely the same treatment. One will be weaker or stronger than another; certain organs will be more affected in one person than in others. Of a number of persons who have lived too freely, and under nearly similar circumstances, for the same number of years, you will find that some live to a good old age; others die early, of some acute disease; many get kidney-disease; some die of a contracted and wasted liver; some lose their nutrition, and get pulmonary affections; others get diseased arteries, and die of aneurism or heart-disease, or of apoplexy resulting from softening of the brain, or of general paralysis. All these conditions, and many others, may result from one cause acting differently in different individuals. Now, there is no one plan of treatment which can be laid down as suitable for every case you may meet with of any particular malady.

It will be more useful to examine most carefully into the nature of all these changes, to consider the circumstances which give rise to them, how they may be retarded, and how life may be prolonged if the organic changes are past reparation, than to hunt out of books a certain number of cases of fever, or bronchitis, or disease of the kidney, tabulate them according to the treatment employed, and use the results you obtain for your future guidance in practice. This last, gentlemen, is not, I need scarcely tell you, the way to make yourselves honest, unprejudiced practitioners, capable of acting with sound judgment in emergencies that will arise. You must observe for yourselves the changes which take place in the patients you see and watch from day to day in the hospital; and you must think how you can influence those changes by altering the conditions under which life is carried on. You must study the influence of heat and cold, diet, and the various

plans of treatment at our disposal, upon the healthy organism, and upon persons whose healthy actions are more or less deranged.

I cannot refrain from noticing some erroneous conclusions, founded upon data which I shall show you are incorrect, that have been lately put forth by an anonymous reviewer, with reference to the results of the treatment pursued by the late Dr. Todd in cases of fever and in other cases of acute disease. I will not allow these numerical statements to go forth to the profession without saying a few words in explanation of the results which have been obtained; and I feel that there is no place in which it would be so appropriate to offer these remarks as this theatre, in the presence of some who, with better opportunities of observing the actual effects of the treatment than the reviewer seems to have had, have watched from day to day the course of many of the cases under Dr. Todd's care.

Anonymous reviewers who commence their observations with expressions of the peculiar sense of responsibility which attaches to their remarks, and proclaim their high respect for the opinions of those with whom they are most reluctantly compelled to differ in every essential point, are perhaps peculiarly liable to imagine that certain statements which really emanate from themselves, and which they proceed straightway to refute by every possible argument, are articles of general belief. These assertions, often quite unimportant in themselves, by a little skilful handling on the part of the reviewer, are at last made to assume proportions sufficiently large to render it worth his while to attempt to demolish them.

It is much to be regretted that this sort of verbose assertion, invented apparently for the mere purpose of subsequent refutation, often forms a considerable part of what is known as a "review"; and there are not a few critics who seem to think it necessary at least to commence in this manner. In many of our journals, articles are admitted in which satire is made to take the place of argument, and sharp writing fills up pages

which should be occupied by honest analysis and temperate discussion. As long as this fashion lasts, we cannot attach much importance to the opinions advanced, or feel confidence in the truth of the facts appealed to by these anonymous writers. Surely an important question of treatment might be fairly criticised by one who was opposed to it, without the necessity for such remarks as "that it has been contended by many of Dr. Todd's disciples that he was the originator of a new system of medicine"; and that "the writer has even heard it stated on more than one occasion that Dr. Todd's views on treatment were more than a century in advance of the age in which he lived." (*Medico-Chirurgical Review*, No. LI, Oct. 1860, p. 325.) Had the author known Dr. Todd, he would have been aware that he frequently referred to those who had employed with success a similar plan of treatment before he commenced practice, and to many of his contemporaries who had been led to the same practice from independent observation, and from their own private experience. Dr. Todd acted upon this plan, as he would have acted upon any other, because he had obtained sufficient evidence to convince himself that it was right. Why too, I would ask, should a writer who has been called upon to review an important work, go far out of his way to annoy his readers by offering what almost amounts to an indirect insult to two distinguished physicians who are not in any way connected with the question at issue?

But, gentlemen, we must not pursue this part of the question further. We may hope that the time is not far distant when medical literature will no longer be degraded with this sort of writing; and I trust you will all uphold, on every opportunity, the propriety of discussing difficult questions freely and earnestly, but calmly and dispassionately, and show that you are not influenced by those who enforce an argument by sarcasm and ridicule, when reasoning fails to convince, and facts are on the opposite side.

The author of this review in one single sentence contradicts

observations based upon careful work, accumulated with anxious care, and confirmed by the statements of several independent witnesses. He sets at nought observations in clinical wards spread over a period of thirty years, and proceeds to deny the truth of the facts upon which a great clinical teacher founded the principles which he taught for nearly a quarter of a century,—principles which have been practically acted upon, and the truth of which has been confirmed in the course of a most active professional career. If the statements of this reviewer be received, the conclusion is forced upon us, that Dr. Todd was totally deceived in the results of his practice, misled by his observations, mistaken in his views, and prejudiced in favour of a doctrine which was not supported by facts; or we must accept another alternative which is only ungenerously hinted at, because it is well known to be contradicted by facts, and to be inconsistent with what is known of Dr. Todd's whole career.

I would ask, was it likely that a man who considered himself to be, and worked till the last at his profession as, a student, was so utterly mistaken in his views, and so completely deceived in the results of his treatment, and wrong in his judgment, as this reviewer concludes? Were those house physicians, and clinical clerks and students, who watched during a period of more than twenty years his practice in this Hospital, and the practitioners in this country who act upon principles similar to those on which he based his practice,—are all these, and they must number many hundreds of some of the most inquiring minds in our profession, so utterly mistaken in their impressions, *that a quick weak pulse becomes slower and stronger under the influence of alcohol; and that delirium increases as a patient's strength fails, and diminishes when his strength is upheld?* Is it likely that these phenomena, which could be observed again and again by any unprejudiced person who worked for six months in the wards, were in reality merely due, as the reviewer suggests, to the accidents of the malady,

and not to the influence of the alcohol? Did the large quantities of alcohol which many of these patients took exert no effect whatever on the pulse?

This gentleman comments on Dr. Todd's conclusions thus: "We have no hesitation in declaring, as the result of considerable investigations into this subject, that the rate of mortality of Dr. Todd's fever cases was much in excess of any other hospital in the United Kingdom?" If this were all, it would not have been necessary to notice the review, as the experience of most readers would prevent them from giving implicit credit to a general statement of this kind, made anonymously. But, armed with the formidable artillery of numbers, he produces facts in support of his assertions which few would be inclined to contradict; and he draws conclusions from the facts which the ordinary reader would not fail to accept. He says that, "about a year before his death, Dr. Todd handed over to him his case-books from the year 1840 to the year 1858, for the express purpose of analysing the results of his treatment."

It is unfortunate that these books should have fallen into the hands of one so well able to analyse, and so unfortunate in his efforts to extract the truth. After making a most laborious analysis, he makes out that the deaths of *typhus, typhoid, and relapsing fever, amounted to 18.29 per cent., while in other hospitals the mortality never exceeded 14 per cent. The deaths of Dr. Todd's cases of typhus fever reached 26 per cent.* Now, I shall have no difficulty in showing you that all this calculation has been worse than useless, because it has caused the reviewer to propagate a most unpardonable error. His analytical work doubtless is right; but his conclusions are utterly wrong, for the simple reason that the facts upon which his calculations were based are incomplete. He *assumes*, what he ought to have ascertained before he began his inquiry, that the case-books he analysed contained reports of *all* the fever cases admitted under Dr. Todd in the above mentioned period. It is hardly credible that so simple a point was not at once determined by

questioning those who were familiar with the manner in which the cases were recorded.

Suppose, gentlemen, that you were carefully to report during the next few years, in your own case-books, cases of fever which had been treated by me in the hospital in the same manner. If you found, on examination, that of one hundred cases of fever, thirty-three died, or the deaths amounted to 33.33 per cent., would you come to the conclusion that you were justified in at once publishing a report of the results of the treatment which you had watched, and proclaiming to the world that the percentage of deaths in my cases was greater than among those under any other practitioner in the United Kingdom; or would it occur to you that it would be well just to ascertain, in the first instance, if by any possibility other cases of the same disease, treated in the same way, had been under my care, and had progressed favourably, although they had not, for some reason, fallen under your notice? If you had published your report, you would have acted in very much the same way towards me as the reviewer has acted towards Dr. Todd. Except that Dr. Todd seems to have forwarded his own case-books to the reviewer for the express purpose of analysis, without having considered the points presently to be mentioned. But it is very extraordinary it did not occur to the statistical inquirer who went through all this labour, that it would be well, in the first place, to make sure that every case had been entered in the books. The author of this review could not have been acquainted with the following important facts:—

1. *That a great number of the slight cases are never entered in the case-books at all.*

2. *That for two months or more, in the summer time, very few cases of any kind are recorded in some years.*

3. *That, as Dr. Todd was well known to take a great interest in fever, probably more of the worst cases than in ordinary course fell to his share, were from time to time placed under him.*

If you add to these causes of error the fact that *our hospital, being a small one in the very heart of a very poor neighbourhood, receives a greater per centage of bad cases than most other institutions, and its death-rate in all cases is accordingly very high; that the rules of admission into the different beds have not been always the same; and that, as the clerks who take the cases change every six months, at the commencement of their terms of office, and towards the close, irregularities occasionally occur,*—you will, I think, agree with me when I say that these case-books, valuable as they are for clinical work and for reference, cannot justly be made the subjects of statistical inquiries as to the results of treatment. The case-books were never arranged for this purpose, and cannot be looked upon as registers of every case admitted.

So far from the results telling against Dr. Todd's plan of treatment, one is surprised, when all the causes of error are considered, that the mortality did not appear much higher; and that the percentage of recoveries in *very bad* cases has so far counterbalanced the numerous sources of error, all tending to cause a very high death-rate, as to produce a result only 4 or 5 per cent. above that of institutions where all such sources of error are excluded.

The calculations of the author of that review may be the most correct that ever were made, and I will give him every credit for the patience and assiduity which he has displayed in drawing up his report; but correctness of calculation, patience, and great labour, may be applied in a wrong direction, as well as in a right one; and here we have an example of a reviewer inflicting, unintentionally, a wrong not only upon the memory of Dr. Todd and on his immediate friends, but upon those who have received their education in this school, as well as upon those who pursue statistical inquiries in various departments of medicine.

Gentlemen, false as well as true conclusions are not unfrequently arrived at in the course of numerical inquiries. The

analyser should carefully sift every point before he commences his observations; he should have no feeling as to the nature of the result he is bringing out by calculation; he should be free from all theories, and determined to obtain the simple facts. A gross misrepresentation may be forced upon us in the course of the soundest argument, and incontrovertible logic may for a time be made to hide the most patent untruth. There are logicians who love logic for its own sake, and care not for the truth of the premises upon which their argument is founded; and there are those who love to tabulate and calculate results, without being careful to take the trouble to investigate the several facts represented by their figures. If you accept the premises, you may be compelled to admit that the decillionth of a grain of gamboge will purge you; and you may be convinced by numbers that cases of pneumonia may be physicked, stimulated, depleted, or let alone, with precisely the same result. Now, you can easily put the truth of the first conclusion to experimental proof, if you think it worth while. You cannot determine the second so easily or so quickly. Nevertheless, just take a column of any fifty cases under the head of "pneumonia", for instance; then examine the cases *seriatim*; and I think it will be difficult to find many which agree sufficiently in all points to enable you to arrive at any conclusions as to the effects of the various remedies employed. Depend upon it, the mode of inquiry is wrong altogether. We shall never learn much of the action of remedies, or of the nature of disease, from working at a large number of cases, unless it were possible to obtain the same sort of people, placed under exactly similar circumstances, to work upon. But we cannot fail to learn a great many new truths, if we very carefully watch and narrowly observe the changes in each individual case that comes under our care. This is, at least in our day, the only way in which we can hope to make ourselves sound practitioners. Each case is a study in itself, and must be regarded from every point of view. We must ascertain actual conditions,

and investigate the various actions going on in the body, instead of trying to find out if the affection is to be called pneumonia or bronchitis, and looking out in our statistical reports to ascertain under what treatment the largest percentage of cases recovered.

I do not propose to enter into a discussion of the question of treatment of acute disease. To discuss the different opinions of various writers would only confuse and perplex you, instead of affording you assistance in the difficult questions of treatment you will ere long be called upon to decide practically for yourselves. I endeavour to state broadly the principles by which I am guided, and which may to some extent serve to guide you; but I refrain from expressing positive opinions on special questions, because I feel that I have not worked long enough to entitle me to do so. From my colleagues, however, you will obtain the results of more extended observation and larger experience than I am able to bestow. But I have a right to state to you the facts I have observed; and I should be wanting in my duty, were I to allow you to be influenced by positive statements which I believe to be founded on incorrect observation or imperfect data. Many former house-physicians are now in practice here and in various parts of the country, and many who have served this office are undoubtedly known to you. Now, I think you will find, upon inquiry, that many of these gentlemen will tell you they have observed the following broad facts. I know that those who served office about the time I did, will support these conclusions, and would agree with me in stating—

1. *That we have seen, not one or two, but numerous cases of acute disease, in which the exhaustion was great and the pulse very weak and quick, progress steadily and favourably towards recovery under the influence of frequently repeated doses of stimulants.*

2. *That we feel confident that, in cases of exhausting diseases, the delirium becomes less, and gradually ceases, while the*

pulse increases in force and diminishes in frequency, under the influence of repeated doses of alcohol.

3. That in low cases of fever, pneumonia, etc., the odour of brandy in the breath is often not to be detected five or ten minutes after it has entered the stomach, while in health it may be smelt for an hour, and often for much longer.

4. We are confident that the mortality of the cases treated in this manner was not greater than in cases we have seen treated elsewhere; and we are convinced that many bad cases which recovered would have been lost, had the stimulants been withheld, or the amount reduced.

Gentlemen, it may appear but weak to advance impressions and feelings against the facts of figures; but I am certain that, if the opinion of all the men who have watched the results of Dr. Todd's treatment in acute disease could be ascertained, it would bring us nearer the truth than any statistics that can be, or have been, obtained. Conclusions derived from statistical inquiries as to treatment are almost useless, because the columns under one name almost necessarily include a number of facts which ought not to be classified together.

As to the very large quantities of stimulants which have been given in our hospital, as appears when the amount per patient is estimated, it must be borne in mind, as a late house-physician has just remarked to me, that so convinced were the resident medical officers generally of the importance of stimulating very freely in desperate cases of fever, that, for the last twenty-four hours of life, much alcohol was given which would perhaps have been saved in some institutions on the ground that the patient was past recovery, and all treatment was quite useless. Now, I think that most of us feel that such a course is only right, because every now and then we do see a fever-patient recover from a state of extreme exhaustion, although we had believed it to be very unlikely, and when many would have considered the case absolutely hopeless. A few such cases have convinced many of us of the importance of the prac-

tical rule, *never to give up a fever-case while there is life* ; the value of a certain amount of brandy being considered as nothing when there is the shadow of the chance of saving a human life.

Now, pray do not conclude, from the observations I have made, that I undervalue the results obtained from statistical inquiries. There is no department of human knowledge which has made greater advance within a comparatively short time than this. Civilised races have probably never received greater blessings than have resulted from this class of inquiries ; and I honestly believe that many, many lives will be saved, and the average duration of life greatly prolonged, when the conclusions deduced from examining vast collections of carefully selected facts shall have been more fully carried out.

Let me, however, strongly insist upon the importance of very careful and honest examination of the complex facts which very often make up each unit of the sums which are analysed. The truth of the conclusion obviously depends upon the truth of every fact taken into consideration ; and, although the calculation may be most carefully carried out, any one set of facts which may be incorrect will necessarily cause the resulting conclusion to be untrue.

It cannot be denied that inquiries of this nature into the results of treatment tend to induce a sceptical turn of mind with reference to the action of remedies in disease generally ; and it seems to me that many writers in the present day are somewhat too apt to underrate the effects of various plans of treatment which really exert an important influence for good.

There are melancholy moralists in our profession, who attribute the altered practice of our day merely to a cyclical change of fashion. They say that the Brunonian system in vogue towards the close of the last century soon gave place to the reign of blood-letting, which is now replaced by the other extreme ; and that this will itself ere long be succeeded by another reaction in favour of depressing systems. I think that men

who argue in this way must be very despondent, very prejudiced, very blind, or very unreasonable. They conclude there is no truth in our principles, and that there are no means which will enable us to arrive at truth; that the prejudices of one school are in the ascendant for a period, and that after a time these give place to prejudices of a totally opposite character; that at one time depletion is pushed to a dangerous extent in cases which half a century later would be nearly destroyed by over-stimulating. Now, I venture to say, you will meet with people who talk in this manner, and you will read the observations of those who think in this despondent, hopeless sort of style; but do not be influenced by such dismal forebodings. There is a brighter and more hopeful picture; there are principles which will guide us; there is truth in medicine; and he must be both melancholy and ignorant who is not convinced that the practice of medicine in the present day is in advance of that of fifty, nay, even twenty years ago. It was the custom to bleed in certain cases of inflammation, *because* it was believed that this process essentially consisted, as its name implies, of a violent exciting action tending to destroy life by the intensity of the oxidising processes; that the increased activity of the organs of respiration and circulation, the redness of the skin, the fever, and, above all, the buffy coat of the blood, were evidences of this. The increase of the fibrine, associated with many of these inflammatory states, it was concluded, was a very essential part of the condition. It was assumed that a quick, rapid pulse was evidence of the presence of a highly inflammatory state, and that delirium was but too often due to inflammation of the brain or its membranes. To relieve this inflammatory tendency, the vital powers must be depressed, the system must be relieved by drawing blood. If the blood coagulated slowly with the formation of a firm buffy coat, and contained an increase of fibrine, the bleeding must be repeated; and if, on the second blood-letting, the same phenomena were observed, it was held that we should bleed again;

and so on till the inflammatory condition was subdued, or the patient had ceased to exist.

It has in late years been observed that these acute inflammations often come on in persons who are weak, whose vital powers are depressed, and whose blood is poor. It has been shown that excess of fibrine occurs also in other conditions of disease, and is no evidence of the so-called inflammatory tendency. It has been demonstrated that the course of inflammation is not necessarily stopped by depressing the vital powers; and it has been proved experimentally, in animals, that the quantity of fibrine in the blood is increased relatively and absolutely by successive bleedings; and that this increase is still greater when the animal is bled and starved at the same time. There is, therefore, surely little ground for the fear that the blood-letting system will ever again be practised as it was forty years ago. There is little justice in the remark that our treatment fluctuates according to periodical changes in fashion. Every year we live, new investigations are added to the knowledge we possess, and from time to time new principles are laid down for our guidance; so that in very much, at least, retrogression seems almost impossible.

But I must not now dilate further upon this very interesting and important question. You will not take a desponding view of medicine, or allow yourselves to be influenced by those who do, if you observe for yourselves, think for yourselves, carefully watch the results of experiment, and work. You will find enough to hope for and to contemplate in your professional labours; and if you push forward honestly, with a determination to be as useful as you can to your fellow-creatures, you will not find the time, nor will you feel inclined, to listen to the dismal forebodings of those who grieve instead of working, and lament over the vanity of medical skill, instead of making an effort to advance knowledge, practical or scientific. Be assured that, if you work and try to think earnestly and vigorously, you will soon see reason to

look forward hopefully to the future of medicine, and you will encourage others to do the same.

Let me now ask your attention while I give you a brief outline of the most important cases under my care. Some of these are very interesting; and you will, I think, gain much practical information by watching them attentively while they remain under treatment.

CASE I. *Enlarged Kidney.* The first case is that of a pale thin woman, about 35 years of age, who is suffering from hysterical symptoms, slight cough, and wandering rheumatic pains affecting the fibrous tissues and muscles in various parts of the body; and now and then one of the larger joints becomes tender and painful. But the most important symptom is general dropsy, depending, as we found upon further examination, upon kidney-disease.

From the *history*, we learn that this patient has already suffered from several attacks of dropsy, none of which have been very severe. Every now and then she takes cold, finds that her urine becomes scanty, and she notices the legs begin to swell. She comes into the hospital; and in the course of a short time, the action of the skin and bowels being excited, while the kidneys are allowed to remain quiet, the renal irritation passes off, the quantity of urine increases, the dropsy very nearly disappears, and the patient leaves the hospital—to be readmitted some few months afterwards, perhaps, with a recurrence of the same symptoms.

She is now recovering; the urine is pale, its specific gravity 1007; and she passes as much as four pints in the twenty-four hours. It still contains a considerable quantity of albumen, and the deposit has been found to contain oil-casts. It is probable that this woman is suffering from that form of Bright's disease which is due to enlarged kidney. If this patient is careful to avoid exposure to cold, and is cautious as to diet, etc., she may, I think, live for some years; but it is not likely that the albu-

men will completely disappear from the urine. Now that she is recovering from this last acute attack, we are giving her a nutritious diet, and she is taking tincture of the sesquichloride of iron.

CASE II. *Lepra*. Close to this patient is a girl 16 years of age, suffering from *lepra*. The skin of the arms and legs was almost covered with this scaly eruption; you may now observe numerous round patches, varying much in size, of a reddish colour, slightly raised, and covered with whitish silvery-looking scales; and you will not fail to notice that many of the larger ones exhibit a patch of healthy skin in the centre. In fact, the disease appears first as a spot, which spreads at its circumference until a circular patch, of perhaps an inch in diameter, is formed; then the skin in the central part begins to heal, while the disease continues to spread at the circumference. You may verify these observations by attentively examining this patient. This is what we may call a bad case of the disease, inasmuch as the girl has suffered for five years, and has only been well once during that interval of time. She is now progressing very favourably, under the influence of the constant application of a weak glycerine lotion (glycerine one, water three parts), a moderate purge, and a liberal diet, without beer. This plan we have seen succeed in several cases. Its usefulness probably depends upon the cuticle being kept moist, which enables it to recover its healthy state of growth. It is important to bear in mind that this form of skin-disease, and indeed almost all forms, are aggravated by beer.

CASE III. *Pelvic Abscess opening into the Rectum and Bladder*. There is another very interesting case in this ward. It is that of a poor woman, aged 40, who about three months since appears to have taken cold, and was soon afterwards seized with vomiting, and a violent shooting pain in the lower part of the abdomen. She was seen by Dr. Armitage, who detected a swelling, which was dull on percussion, in the right inguinal region; this he considered to be the corresponding ovary en-

larged from acute inflammation. She was admitted into the hospital, and, about three weeks after the commencement of her illness, we found a considerable quantity of pus in her urine, and we noticed that the swelling, which up to this time had somewhat increased, had almost disappeared. A fortnight later, she passed pus by the bowel. This state of things still continues—pus passing sometimes from the bladder, sometimes from the bowel. There can be no doubt that in this case there is an abscess, probably of the ovary, which has opened into the rectum on the one hand, and into the upper part of the vagina, or into the bladder, on the other.

CASE IV. *Suffocative Catarrh.* Close to this patient there is a girl, aged 16, suffering from what is sometimes called suffocative catarrh. The lungs are emphysematous; the right side of the heart is enlarged; the pulmonary vessels and the systemic veins are gorged with blood. The distress of breathing a few days since was very great; pulse 152; respiration 50. We tried several remedies, and counter-irritation, without benefit; but she was immediately relieved by cupping over the lower part of each lung behind. An ounce of blood was removed in each glass. She has been taking chloric ether and ammonia, and is now nearly convalescent.

In the men's ward we have a very important case of dropsy, consequent upon an acute attack of pericarditis with effusion, uncomplicated with any other malady, and not associated with rheumatism. Every one should have notes of this case; for it is an example of the results which follow adhesion of the pericardium and enlargement of the heart, in a man whose organs generally were quite healthy.

CASE V. *Acute Pericarditis with Effusion, Unconnected with Rheumatism.* The patient is a man who was quite strong and healthy, and never suffered from any illness previously, and never complained of rheumatic pains. He was a soldier, 21 years of age. Early in January of the present year he caught cold, and about ten days afterwards was admitted into the hos-

pital with a loud to-and-fro rubbing sound. This very soon ceased; but the distress of breathing greatly increased; the pulse became very small and irregular; and the cardiac dulness was very much extended, as shewn in this outline. In about a week the rubbing sound reappeared, the extent of dulness became more limited, and the patient afterwards convalesced rapidly. He left the hospital in a month, with adherent pericardium. Three weeks afterwards he was again admitted, with the pericardium distended with fluid. Under the influence of opium, mercurial inunction, and a supporting diet, he gradually recovered a second time. Now he is again an in-patient, with enlarged heart and obstructed pulmonary circulation, general venous congestion, considerable ascites, and œdema of the legs. The liver is somewhat enlarged, and probably all the internal organs are much congested with venous blood. He makes only a few ounces of urine in twenty-four hours; and his sclerotics and skin are sallow. All these symptoms probably result from the interference with the proper action of the heart caused by the adherent pericardium. You must bear in mind that this is one of those very rare cases in which we meet with very serious pericarditis *not* connected with rheumatic fever. But we must go more at length into this man's case in another lecture.

There are three other cases, upon which I hoped to have said a few words; but these must also be deferred to another opportunity.



